

intelligence. In the first place, what the writer calls primary instincts, including those of many low animals and certain instincts of higher animals, *e.g.* incubation, arise by the action of the first cause. This is proved by the fact that purposeless habits, tricks of manner, *e.g.* the trick of barking round a carriage showing itself in certain varieties of dogs, occur and are inherited. In the second place, secondary instincts, including many of those of the higher animals, *e.g.* dread and shunning of man, or other enemies, were originally intelligent actions, and illustrate the principle of habit or lapsed intelligence. This proposition, again, is established by showing first, that "intelligent adjustments when frequently performed become automatic in the individual, and next that they are inherited till they become automatic habits in the race," *e.g.* in the tendency of certain breeds of dogs to "beg."

In combining both these principles in his theory of instinct, Mr. Romanes follows his master, Mr. Darwin, and he has derived much assistance from the valuable essay on instinct by that writer, which was written for the "Origin of Species," but, having been withheld from that publication for want of space, now appears for the first time as an appendix to Mr. Romanes' volume. But the author has elaborated the theory sketched out by Mr. Darwin. More particularly he has illustrated at great length how the two causes may combine. He shows how on the one hand, primary instincts may come to be put to better uses by intelligence, and, on the other hand, secondary instincts may be modified and put to better uses by natural selection. The effects of domestication illustrate most clearly this conjoint action of the two principles. With respect to the comparative importance of the two causes, Mr. Romanes seems inclined to look at natural selection as the chief agency, intelligent adjustment being regarded as an auxiliary agency, the chief function of which is to supply to the controlling principle of natural selection an additional class of variations which are from the first adaptive. Mr. Romanes supports his theory by a cumulative chain of argument of very great strength, and he orders the successive steps of it in such a way as to make the reader feel its full force. His main positions seem to us unassailable. The only point we feel inclined to criticise is the limitation of the action of intelligence in the instincts of animals low down in the scale. The author appears to argue on general grounds that these must to a large extent be due to the working of natural selection. But the facts of intelligent modification of instinctive actions cited by him, *e.g.* in the case of the constructive actions of bees, &c., appear to show that the animals concerned possess a considerable measure of genuine sagacity. And while it is no doubt difficult, as the author remarks (p. 191), to attribute to an animal so low down in the scale as the larva of the caddice fly a power of consciously reasoning, it seems, on the other hand, hard to understand how, by the mere play of natural selection unaided by any rudiment of conscious discrimination and adaptation of means to ends, this little creature could have acquired the habit of either lightening its floating case by attaching a leaf to it or weighting it by attaching a small stone according as it becomes too heavy or too light. But the author shows himself so completely the master of his subject, that the reader feels

disposed to accept his conclusions in the very few instances in which his individual judgment leans the other way.

JAMES SULLY

OUR BOOK SHELF

An Introduction to the Study of Heat. By J. Hamblin Smith, M.A. (London: Rivingtons, 1883.)

THOUGH the author states in the preface that "he has endeavoured in this book to explain the elementary facts connected with the theory of heat so far as a knowledge of them is required by the University of Cambridge in the general examination for the ordinary B.A. degree," it will be found that he has succeeded in producing a book which is not only admirably adapted to help a student who is preparing for this or any other elementary examination, but which, from the simple nature of the language and the clearness of the descriptions, may be read with advantage by those who have no examination to pass, but who may wish to understand something of the science of heat for its own sake.

The text is composed of short numbered paragraphs, in each of which the author deals with one new fact only, a plan eminently adapted to save the student confusion. These paragraphs may be taken as model answers to imaginary examination questions.

Over two hundred questions are given on those parts of the subject, such as expansion, calorimetry, conductivity and hygrometry, which admit of being put in simple numerical form. Many of these are essentially exercises in arithmetic, and must irresistibly remind the reader of the unlikely questions which he used to have to answer at school. In the questions on thermometers, for instance, an observer seems to have noted the sums, differences, products, &c., of the readings of every kind of thermometer in his laboratory, without noticing what those readings were, and then, when too late, to have met with the necessity of finding from his observations the temperatures which the instruments actually indicated. However, though observations of such a kind are rarely made, the exercises which they furnish will of necessity make those who work them out absolutely familiar with the fundamental principles of the subject.

C. V. B.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Ear a Barometer

AT a time when I frequently went between Peterborough and London by the Great Northern Railway express trains, I found that the sudden compression of the air produced on entering a tunnel was not only perceptible by the ear, but even unpleasant, and that this unpleasant sensation remained till the open air was reached, when it suddenly ceased. Of course it was natural to suppose that the noise was the primary cause, but I satisfied myself that this had nothing to do with the effect, for on swallowing after entering the tunnel the sensation ceased, but recurred in the opposite sense on leaving the tunnel, when a second operation of swallowing removed it. This showed clearly that what was observed was real.

As far as I remember there was, as measured by the sensation, an increase of pressure, at first sudden, and then gradually rising for a second or two on entering, and a corresponding gradual and sudden decrease on leaving a tunnel.

I did not at the time have the opportunity of taking an aneroid with me to measure the amount of the compression, but intended to try an air thermometer which I thought would be more

sensitive to a sudden change of pressure than even the most delicate aneroid.

It is strange that so few people have noticed this sensation in the ears; besides Mr. Horace Darwin I do not know of any one who I may say has been disturbed by it.

During the gale on the 22nd and 23rd inst. it occurred to me to try whether I could get an idea of the pressure that could be felt by the ear. My room faces west, and the wind was beating against the windows; so after shutting the door I opened one of the windows suddenly during a furious gust, at which a small gas-flame bobbed nearly out, and the same instantaneous sensation was plainly felt. On shutting it again the gas-flame started up, but the inverse sensation was not perceptible. The gas-flame stood at apparently the same height with the window shut or open. Its movements were simply due to the starting and stopping of an extra draught in the chimney. On examining the meniscus at the top of the column in a mercurial barometer, the change of pressure was plainly visible, not by a rise and fall of the whole column, but by a change of curvature which was very marked when the window was opened during the stronger gusts. A pocket aneroid showed the same thing perfectly, rising, as far as I could judge, about $1/150$ inch in general, but during one very furious blast $1/20$ inch; on that occasion only did the shock on the ears seem at all comparable with what I remember to have felt in the tunnels.

It is not necessary to wait for a gale in the right direction to test the ears. I found that if a friend charged the door with his whole strength, much the same compression was produced as by the average gusts of wind. Of course the compression will depend partly on the contents of the room, which were, in the case in question, about 2500 cubic feet.

It is probable that the change of pressure noticed by the ear is greater than that shown by the barometer, for in instantaneous effect on the gas flame was enormous, while the permanent action was barely perceptible; on the other hand, the aneroid showed a permanent displacement with only a very slight recoil. The greater mobility of the gas doubtless corresponds to the great sensibility of the ear.

If the actual change of pressure felt by the ear is $1/150$ inch, which corresponds to a change of level of six feet, it might be expected that a sensation would be observed on running up or down stairs. This I have not noticed, the change of pressure being so gradual.

I need hardly add that descending a mine at the high speeds common in the collieries is most painful to me, and is only rendered bearable by continuously swallowing.

The very great and apparently unrecognised difference that there seems to be in the sensibility of the ears of different individuals may be an excuse for occupying so much of your valuable space with what is in other respects a long and uninteresting letter.

C. V. Boys

Physical Laboratory, South Kensington

The Remarkable Sunsets

IN NATURE for December 20, 1883, Dr. James Macaulay has collected (pp. 176, 177) some recorded instances of the wide distribution at former periods of volcanic dust. Perhaps the following may be worth adding. It is to be found in that extraordinary repertory of curious information and suggestion, the "Philosophical Notes" to Darwin's "Botanic Garden" (part ii. 3rd edition, 1791, p. 167).

W. T. T. D.

The Rev. Mr. Sterling gives an account of a darkness for six or eight hours at Detroit in America, on October 19, 1762, in which the sun appeared as red as blood, and thrice its usual size: some rain falling covered white paper with dark drops like sulphur or dirt, which burnt like wet gunpowder, and the air had a very sulphureous smell. He supposes this to have been emitted from some distant earthquake or volcano (*Phil. Trans.* v. liii. p. 63).

In many circumstances this wind [the Haimattan] seems much to resemble the dry fog which covered most parts of Europe in the summer of 1780, which has been supposed to have had a volcanic origin, as it succeeded the violent eruption of Mount Hecla and its neighbourhood. From the subsidence of a white powder, it seems probable that the Haimattan has a similar origin, from the unexplored mountains of Africa. Nor is it improbable that the epidemic coughs which occasionally traverse immense tracts of country may be the products of volcanic eruptions; nor impossible that at some future time contagious

miasma may be thus emitted from subterranean fissures in such abundance as to contaminate the whole atmosphere and depopulate the earth (Darwin's "Botanic Garden," part ii. 3rd edition, 1791, p. 167).

WE had the sunset display again to-night, but the after-glow was much less prolonged, suggesting that the stratum of dust and crystals is slowly settling down. But repeated flashes of lightning and peals of thunder, in a place where storms, at the usual time of year for them, are very infrequent, seem to suggest also the question whether the ash is not brought within the sphere of rain-clouds rather by loss of electricity than the influence of surface-gales. At the same time the large fluctuations of pressure seem to tell, on the contrary, that the whole column is affected to unusual altitudes. Since I have been a reader of journals I have seen nothing more entralling in its interest than the contributions made, week by week, to NATURE on this subject from all parts of the world. It marks an era in observation from which we may hope great things in the future.

Bregner, Bournemouth, February 2

HENRY CECIL

REFERRING to the latter part of Mr. Hawell's letter in NATURE, January 24 (p. 285), there seem to be several different ways in which volcanic dust might affect the temperature, and though all of them seem likely to have but small effect, the quantities they affect are so vast that a very small percentage may form a very considerable quantity.

1. The volcanic gases would form at first a stratum much warmer than would be natural to the heights at which they would rest, and would thus retard the outward flow of heat from the earth.

2. The volcanic dust, forming an unusually high stratum of opaque matter, would intercept rays from the sun that would be otherwise lost to the earth.

3. The volcanic dust would act as a screen to prevent the earth losing heat by radiation, while it would also (4) act as a screen to prevent the sun's rays reaching the earth; but in so doing would make the dust stratum warmer, and so would aid cause No. 1.

The indirect effects, as influencing evaporation and condensation, and the formation of clouds, are probably greater than the direct, but are more difficult to analyse.

December 3.—I have noticed that December 3 was generally remarkably cloudy. Here, however, it was conspicuous as the day on which all the most marked features of the sunsets culminated. At 4.45 the green and pink glows covered the western half of the sky, and the rest of the sky was filled with a purple glow of like character, while the crescent moon was green. These glows had to a great extent faded at 5; and though the phenomenon lasted late, I can give no more detail, as I took no notes, not being able to give it continuous attention.

37, The Square, Ripon, January 28

W. W. TAYLOR

Christian Conrad Sprengel

SPEAKING of Christian Conrad Sprengel's discoveries, Dr. H. A. Hagen says (NATURE, vol. xxix. p. 29):—"In Germany these discoveries were well known to every naturalist during the whole century. Certainly between 1830 and 1870 at every university in Prussia the same facts were taught as well-known facts of the highest importance, and of course known by every student." From the complete want of papers relating to the facts observed, and the theories proposed by Sprengel in the German botanical and entomological periodicals published before the time of Darwin, strangely contrasting with the profusion of such papers in modern botanical literature, one might have been led to a very different conclusion, viz. that Sprengel had fallen into almost complete oblivion in Germany also, and that hardly any professor in any of the universities of Prussia and of Germany in general duly appreciated and taught his discoveries before Darwin's time. And this, I think, is really the case. Certainly at the University of Berlin in 1841, neither Lichtenstein, in his lectures on zoology, nor Kunth in those on botany, ever spoke of Sprengel and his work, nor did Erichson in his course on entomology. At the University of Greifswald, in 1842, the professor of natural history, Hornschuch, never mentioned Sprengel's discoveries. In 1848 my brother, Hermann Müller, began the study of zoology and botany at the University of Halle, where he never heard of Sprengel, with whose work